200 cases, the advantages of this approach over sedation techniques are, I suggest:

A single visit to the hospital regardless of the child’s ‘cooperation level’.

No necessity for awake, tearful needles.

Predictably rapid time to readiness and scan time—optimal scanner utilization.

Guaranteed completion of high diagnostic quality scans with minimal movement artifact.

Safety for unconscious children in a remote setting.

I therefore disagree with their concluding paragraph. ‘Efficiency’ is not the sole reason why so many units prefer general anaesthesia. I suspect ‘quality’ is the main consideration.

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Editor—Thank you for the opportunity to respond to Dr Allen’s comments. Quality, defined as how good or bad something is, is a matter of opinion. Many parents prefer sedation delivered by mouth—provided it is successful—because their children dislike both inhalational and i.v. inductions. Neither the time taken for scan readiness, nor scanner utilization are quality issues for individual patients; indeed they are both measures of efficiency. Nevertheless I accept that a single visit to gain predictable anaesthesia and perfect imaging is a higher quality experience than difficult or failed sedation (not forgetting that anaesthesia inductions can also be difficult). The safety of a particular technique is largely dependent upon the practitioner. Sedation should be safe enough provided the judgement and skills of the sedationist are satisfactory. I think it is highly unlikely that there are good data to support the widely held assumption that anaesthesia is safer than sedation for MRI in children. Certainly, in our experience of sedating over 6000 children for MRI, we have not had a serious airway incident whereas I doubt that I could anaesthetize a similar cohort without there being several potentially life-threatening cases of laryngospasm. It is important to emphasize that our children were selected—that is sedated children are healthier than those selected for anaesthesia. Notwithstanding all these thoughts, most hospitals will use anaesthetists for MRI if they have enough of them.

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Emergence and recovery in children after desflurane and isoflurane anaesthesia

Editor—I wish to comment on the interesting study by Nordmann and colleagues in which isoflurane and desflurane anaesthesia were compared with respect to various indices of the speed of emergence, and in particular the ‘exposure-time’ sensitivity of these indices. Pharmacokinetic and mathematical models predict that emergence following desflurane would be quicker than that following isoflurane and this study usefully provides experimental confirmation of the hypotheses underlying these models.

The authors state that younger children are less susceptible to delayed emergence following prolonged isoflurane exposure as compared with older/larger children. The evidence for this is not clear. Their Figures 4 and 5 show a regression plot of ‘time to extubation’ vs ‘duration of anaesthetic’, and for isoflurane the slope appears to be smaller for children <4 yr compared with >4 yr. No statistical analysis for this comparison is presented, so it is not clear how the assertion is justified. Moreover, given that the data are non-parametric I am not sure that linear regression analysis is appropriate anyway. Whilst it is not my intention to criticize statistics, Figure 4 particularly merits comment. It shows (to the naked eye) considerable scatter of points. Non-parametric data of this sort have characteristic outliers which defy analysis with linear regression (note the high outlier for isoflurane which will weight the linear regression slope). In addition, the ‘range’ of exposure-times is greater for isoflurane. Only the isoflurane group has data recorded for durations over 110 min. Desflurane data points do not have a narrower time-span, and so with this degree of y-axis scatter bunched around a small x-axis span, finding a regression coefficient around zero is more likely. This is not to say that I doubt the effect (I do not), but this analysis does not seem to show it unequivocally.

The authors offer an explanation as to why younger children are less susceptible to delayed emergence following prolonged exposure as compared with older/larger children. They suggest that this may be attributable to the larger tidal-volume:FRC ratio (i.e. ventilatory rate-constant) in younger children resulting in ‘more rapid exchange in alveolar gas, and therefore a faster elimination of the volatile agent’ Mathematically this does not make sense. Certainly if we consider the non-steady state of an exponential wash-out (albeit achieved tidally) of a completely insoluble gas contained in the alveolar compartment, then reducing FRC for a given ventilation rate would indeed speed-up the wash-out. This might explain to some degree any differences between small and large children when considering speeds of wash-in or wash-out following short exposures (although this was not explicitly shown), but it cannot explain the ‘exposure-time’ sensitivity alone. The reason for this is that during emergence following prolonged exposure, the model moves away from that resembling exponential

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wash-out of helium from the alveolar compartment, to one more akin to steady-state (albeit drifting) alveolar elimination of anaesthetic agent delivered to the lung from the tissues via the blood. This more resembles the process of normal CO₂ elimination than helium wash-out. Clearly increasing the VT:FRC ratio has no effect on CO₂ elimination, which is dependent merely on the alveolar ventilation rate.

Finally, the authors ascribe the observed effects to the differences in the blood-gas partition coefficient between the two agents. Whilst this is certainly a dominant factor (particularly in wash-in and wash-out after a short exposure) it does not wholly explain exposure-time dependency (particularly as this characteristic is proposed to differ between small and larger children, yet the blood-gas solubilities do not differ between these groups).

There are two distinct processes underway during emergence: one is elimination of vapour from the alveolar compartment and the other is elution of agent from the tissues and subsequent transfer to the alveolar compartment. The dominance (and rate-limiting effect) of one process over the other varies, and is key to overall elimination. Following prolonged anaesthesia, the elimination characteristics move away from the ‘non-steady-state helium wash-out’ type of kinetics to the ‘drifting-steady-state’ type, where alveolar elimination equals (or just exceeds) tissue elution rate. The effect of blood-gas solubility is less dominant here.

Mapleson’s 1973 model² of volatile agent kinetics is complex, but looked at very simply; there is a large reservoir of volatile agent; notably in fat. The capacity of this reservoir for an agent depends not on the agent’s blood-gas solubility, but on its lipid solubility. The rate-constant for the wash-out (by the blood) of agent from the reservoir is a function of the volume of the reservoir, and the ratio of the ‘blood-gas’ to ‘tissue-gas solubility’. Desflurane’s blood-gas solubility is 3-fold less than isoflurane, but its oil-gas solubility is 5-fold less than isoflurane. Hence the reservoir’s wash-out rate-constant is greater for desflurane than for isoflurane.

So in summary, during emergence from desflurane anaesthesia, the lack of ‘exposure-time’ dependency is as much (or more) a function of its low potency (i.e. low lipid solubility) as its low blood-gas solubility.

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Editor—We would like to thank Dr Farmery for his interest in our study. We recognize that linear regression analysis assumes the data are Gaussian and are not particularly appropriate for non-parametric data. We therefore did not carry out any further statistical tests from Figures 4 and 5 or refer to them with a view to statement justification. All statistical tests were non-parametric and are summarized in the second paragraph of the Results section. From these data, we can see, first, that times to reach recovery milestones for all age groups were similar for desflurane. Although a significant difference was discovered in these milestones between desflurane and isoflurane for both age groups the younger patients (<4 yr) had markedly lower median times to all recovery milestones. It is from this we observed that the younger children seemed to be less susceptible to delayed emergence from isoflurane compared with older children.

We thank Dr Farmery’s further suggestions as to the possible physiological and pharmacological mechanisms involved in explaining the reduced time effects of isoflurane on emergence from anaesthesia in younger children are helpful. Our view, based on the known age-related differences in physiology is that faster wake-up is primarily related to younger children having a higher alveolar volume:FRC ratio, a larger cardiac index and a greater relative blood flow to the brain in the younger age group. These features will all act to speed both onset of anaesthesia and emergence.

We are in total agreement that the lack of lipid solubility of desflurane is the dominant factor for its more rapid emergence and recovery characteristics. Our data support the theoretical basis that the physicochemical characteristics of desflurane result in emergence from anaesthesias that is influenced minimally by the duration of anaesthesia (context insensitive).